parallel straight lines, A and B, and we introduce such a relation between x_0, y_0, x, and y as will express that when the former is a point on the line A the latter will be a point on B, each of the four quantities x, y, u, v may then be expressed as a function of x_0, y_0, u_0, v_0, and it may be proved that

$$dx\,dy\,du\,dv = \frac{V_0}{V} dx_0\,dy_0\,du_0\,dv_0\,;$$

where V_0 and V are the resolved parts of the projectile's velocity perpendicular to the two lines as it crosses A and B respectively.

For instance, let the lines be vertical $x = a$ and $x = b$, where $b - a = c$. Our equations are—

$$x - x_0 = c = u_0 t$$

$$y - y_0 = v_0 t - \frac{gt^2}{2}$$

$$v = v_0 - gt$$

$$u = u_0$$

$$\therefore\ t = \frac{c}{u_0},\ v = v_0 - \frac{gc}{u_0},\ u = u_0,\ x = x_0 + c,\ y = y_0 + \frac{v_0 c}{u_0} - \frac{gc^2}{2u_0^2},$$

and

$$\Delta = 1 = \frac{u}{u_0}.$$

Also here $t = \frac{c}{u_0}$ is *not* constant, as it depends upon u_0.

Next let the lines be horizontal, $y = a, y = b, b - a = c$. We then have

$$(1)\ y - y_0 = c = v_0 t - \frac{gt^2}{2}$$

$$(2)\ x - x_0 = u_0 t$$

$$(3)\quad u = u_0$$

$$(4)\quad v = v_0 - gt.$$

From (1)

$$t = \frac{v_0 - \sqrt{v_0^2 - 2cg}}{g},\qquad x = x_0 + \frac{u_0}{g}\left(v_0 - \sqrt{v_0^2 - 2cg}\right),$$

$$y = y_0 + c,\qquad u = u_0,\qquad v = \sqrt{v_0^2 - 2cg},$$

and our determinant Δ is

$$\begin{vmatrix} 1, & 0, & \frac{v_0 - \sqrt{v_0^2 - 2cg}}{g}, & \frac{u_0}{g}\left\{1 - \frac{v_0}{\sqrt{v_0^2 - 2cg}}\right\} \\ 0, & 1, & 0, & 0 \\ 0, & 0, & 1, & 0 \\ 0, & 0, & 0, & \frac{v_0}{\sqrt{v_0^2 - 2cg}} \end{vmatrix}$$

$$= \frac{v_0}{v}.$$

If our lines were $y = mx$ and $y = mx + c$, our additional condition would be

$$y - y_0 = m(x - x_0) + c\,;$$

and the result mentioned could be arrived at, although with a little additional work.

The actual problem proposed by Boltzmann is the same as this in principle, although of much greater complexity, and it is treated by him with the utmost generality. The important thing here is to show that the S function with t constant is of no application, inasmuch as in both of these very simple illustrations we have t a dependent variable depending upon u_0 or v_0.

I am only pointing out that the S method, with t independent, would not help to establish the particular proposition to which I am referring. It may lead to the determination of a law of permanence of distribution independently of this proposition and by a simpler treatment. The Boltzmann treatment, however, avoids the difficulty which may arise from the fact that *encounters*, whether of finite or infinitely short duration, involve the assumption of discontinuous forces, and, therefore, of a corresponding discontinuity in the form of the S function.

A little consideration shows that the condition E constant cannot lead to any determinate relation between the differential products $dp_1 \ldots dq_n$ and $dP_1 \ldots dQ_n$.

For to take again the simple case of the projectile. Here we get four equations between the nine quantities, $x_0\ y_0\ u_0\ v_0$, $x\ y\ u\ v$ and t, whence it is clear that the elimination of t does not enable us to arrive at more than three equations between the remaining eight quantities, and therefore that we cannot express x, y, u, v separately as determinate functions of $x_0\,y_0\,u_0\,v_0$. To enable us to do this we need one additional condition, and this may be supplied in an infinite number of ways. It may be one of the conditions above considered leading to the equation $dx\,dy\,du\,dv = \frac{V_0}{V} dx_0\,dy_0\,du_0\,dv_0$, or it may be the condition t constant leading to the equality of these differential products, and so forth; but the condition E constant supplies no additional relation between the eight variables. This conclusion holds equally for n degrees of freedom, following from the two partial differential equations in $q_1 \ldots q_n$, $Q_1 \ldots Q_n$, to which the characteristic function A is subject, so that the condition E constant leads to no determinate relation between the differential products.

This conclusion is not inconsistent with Maxwell's proof. That proof takes the form—

$$dp_1 \ldots dq_n = \frac{\Delta}{\Delta'} dP_1 \ldots dQ_n,$$

where Δ is equal to Δ', but it may be proved that in this case Δ and Δ' are separately zero, and therefore that, as stated above, no relation can be established between the two differential products.

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Palæonictis in the American Lower Eocene.

PALÆONTOLOGISTS will welcome Dr. T. L. Wortman's discovery of a nearly complete skull of *Palæonictis* in the Wahsatch Lower Eocene of Wyoming. The only specimens of this form known hitherto are the two fragmentary lower jaws from the Suessonian lignites of France upon which De Blainville founded the genus in 1841. This specimen includes the facial region of the skull and the complete lower jaws in fine preservation. We owe it to the expert skill of Dr. Wortman, for the fossil was found completely dissociated; he carried several sacks of the *débris* surrounding the fragments fifteen miles to the nearest river, and by careful washing recovered all the teeth.

The skull is about the size and form of that of the Puma (*Felis concolor*), without the long muzzle so characteristic of all the early Carnivores or Creodonts. The dental series is remarkably compressed and reduced, especially in the upper jaw, the formula being: I $\frac{3}{3}$, C $\frac{1}{1}$, P $\frac{4}{4}$, M $\frac{2}{2}$. The third upper molar has entirely disappeared, the second is as small as the little tubercular in the modern cats, the first is smaller than the fourth premolar. The latter tooth, in conjunction with the first true lower molar, is in course of transformation into a *sectorial*. This and many other features point to the conclusion that *Palæonictis* is closely related to the Eocene ancestors of the Felidæ—which have hitherto been considered a gap in the fossil series.

The type, which we may call *P. occidentalis*, will soon be fully figured and described. HENRY F. OSBORN.

American Museum of Natural History, April 19.

WATERSTON'S THEORY OF GASES.

ON the 11th of December, 1845, a paper by Mr. J. J. Waterston, entitled "On the Physics of Media that are composed of Free and Perfectly Elastic Molecules in a State of Motion," was communicated by Captain Beaufort, R.N., to the Royal Society.

This paper was not published at the time, but was relegated to the Archives. It now, however, has just been issued as a part of the current volume of Philosophical Transactions.

It is preceded by an introduction by Lord Rayleigh, one of the Secretaries of the Royal Society, and we cannot do better—in order to call attention to this remarkable paper, which anticipates the present theories in many respects, and to explain how it is that it now appears—than print Lord Rayleigh's introduction as it stands, and also the introduction to the memoir itself.

"*Introduction by Lord Rayleigh, Sec.R.S.*

"The publication of this paper after nearly half a century demands a word of explanation; and the opportunity may be taken to point out in what respects the received theory of gases had been anticipated by Waterston, and to offer some suggestions as to the origin of certain errors and deficiencies in his views.

"So far as I am aware, the paper, though always accessible in the Archives of the Royal Society, has remained absolutely unnoticed. Most unfortunately the abstract printed at the time (Roy. Soc. Proc., 1846, vol. v. p. 604; . . .) gave no adequate idea of the scope of the memoir, and still less of the nature of the results arrived at. The deficiency was in some degree supplied by a short account in the Report of the British Association for 1851 (. . .), where is distinctly stated the law, which was afterwards to become so famous, of the equality of the kinetic energies of different molecules at the same temperature.

"My own attention was attracted in the first instance to Waterston's work upon the connection between molecular forces and the latent heat of evaporation, and thence to a paper in the *Philosophical Magazine* for 1858, 'On the Theory of Sound.' He there alludes to the theory of gases under consideration as having been started by Herapath in 1821, and he proceeds:—

"'Mr. Herapath unfortunately assumed heat or temperature to be represented by the simple ratio of the velocity instead of the square of the velocity—being in this apparently led astray by the definition of motion generally received—and thus was baffled in his attempts to reconcile his theory with observation. If we make this change in Mr. Herapath's definition of heat or temperature, viz. that it is proportional to the *vis viva*, or square velocity of the moving particle, not to the momentum, or simple ratio of the velocity, we can without much difficulty deduce, not only the primary laws of elastic fluids, but also the other physical properties of gases enumerated above in the third objection to Newton's hypothesis. In the Archives of the Royal Society for 1845-46, there is a paper "On the Physics of Media that consist of Perfectly Elastic Molecules in a State of Motion," which contains the synthetical reasoning upon which the demonstration of these matters rests. The velocity of sound is therein deduced to be equal to the velocity acquired in falling through three-fourths of a uniform atmosphere. This theory does not take account of the size of the molecules. It assumes that no time is lost at the impact, and that if the impact produce rotatory motion, the *vis viva* thus invested bears a constant ratio to the rectilineal *vis viva*, so as not to require separate consideration. It also does not take account of the probable internal motion of composite molecules; yet the results so closely accord with observation in every part of the subject as to leave no doubt that Mr. Herapath's idea of the physical constitution of gases appproximates closely to the truth. M. Krönig appears to have entered upon the subject in an independent manner, and arrives at the same result; M. Clausius, too, as we learn from his paper "On the Nature of the Motion we call Heat" (*Phil. Mag.*, vol. xiv., 1857, p. 108).'

"Impressed with the above passage and with the general ingenuity and soundness of Waterston's views, I took the first opportunity of consulting the Archives, and saw at once that the memoir justified the large claims made for it, and that it marks an immense advance in the direction of the now generally received theory. The omission to publish it at the time was a misfortune, which probably retarded the development of the subject by ten or fifteen years. It is singular that Waterston appears to have advanced no claim for subsequent publication, whether in the Transactions of the Society, or through some other channel. At any time since 1860 reference would naturally have been made to Maxwell, and it cannot be doubted that he would have at once recommended that everything possible should be done to atone for the original failure of appreciation.

"It is difficult to put oneself in imagination into the position of the reader of 1845, and one can understand that the substance of the memoir should have appeared speculative, and that its mathematical style should have failed to attract. But it is startling to find a referee expressing the opinion that 'the paper is nothing but nonsense, unfit even for reading before the Society.' Another remarks 'that the whole investigation is confessedly founded on a principle entirely hypothetical, from which it is the object to deduce a mathematical representation of the phenomena of elastic media. It exhibits much skill and many remarkable accordances with the general facts, as well as numerical values furnished by observation. . . . The original principle itself involves an assumption which seems to me very difficult to admit, and by no means a satisfactory basis for a mathematical theory, viz. that the elasticity of a medium is to be measured by supposing its molecules in vertical motion, and making a succession of impacts against an elastic gravitating plane.' These remarks are not here quoted with the idea of reflecting upon the judgment of the referee, who was one of the best qualified authorities of the day, and evidently devoted to a most difficult task his careful attention; but rather with the view of throwing light upon the attitude then assumed by men of science in regard to this question, and in order to point a moral. The history of this paper suggests that highly speculative investigations, especially by an unknown author, are best brought before the world through some other channel than a scientific Society, which naturally hesitates to admit into its printed records matter of uncertain value. Perhaps one may go further, and say that a young author who believes himself capable of great things would usually do well to secure the favourable recognition of the scientific world by work whose scope is limited, and whose value is easily judged, before embarking upon higher flights.

"One circumstance which may have told unfavourably upon the reception of Waterston's paper is that he mentions no predecessors. Had he put forward his investigation as a development of the theory of D. Bernoulli, a referee might have hesitated to call it nonsense. It is probable, however, that Waterston was unacquainted with Bernoulli's work, and doubtful whether at that time he knew that Herapath had to some extent foreshadowed similar views.

"At the present time the interest of Waterston's paper can, of course, be little more than historical. What strikes one most is the marvellous courage with which he attacked questions, some of which even now present serious difficulties. To say that he was not always successful is only to deny his claim to rank among the very foremost theorists of all ages. The character of the advance to be dated from this paper will be at once understood when it is realized that Waterston was the first to introduce into the theory the conception that heat and temperature are to be measured by *vis viva*. This enabled him at a stroke to complete Bernoulli's explanation of pressure by showing the accordance of the hypothetical medium with the law of Dalton and Gay-Lussac. In the second section the great feature is the statement (VII.), that 'in mixed media the mean square molecular velocity is inversely proportional to the specific weight of the molecules.' The proof which Waterston gave is doubtless not satisfactory; but the same may be said of that advanced by Maxwell fifteen years later. The law of Avogadro follows at once, as well as that of Graham relative to diffusion. Since the law of equal energies was actually published in 1851, there can be no hesitation, I think, in attaching Waterston's name to it. The attainment of correct results in the third section, dealing with adiabatic expansion, was only prevented by a slip of calculation.

"In a few important respects Waterston stopped short. There is no indication, so far as I can see, that he recognized any other form of motion, or energy, than the translatory motion, though this is sometimes spoken of as vibratory. In this matter the priority in a wider view rests with Clausius. According to Waterston the ratio of specific heats should be (as for mercury vapour) 1·67 in all cases. Again, although he was well aware that the molecular velocity cannot be constant, there is no anticipation of the law of distribution of velocities established by Maxwell.

"A large part of the paper deals with chemistry, and shows that his views upon that subject also were much in advance of those generally held at the time.

"The following extract from a letter by Prof. McLeod will put the reader into possession of the main facts of the case:—

"'It seems a misfortune that the paper was not printed when it was written, for it shadows forth many of the ideas of modern chemistry which have been adopted since 1845, and it might have been the means of hastening their reception by chemists.

"'The author compares the masses of equal volumes of gaseous and volatile elements and compounds, and taking the mass of a unit volume of hydrogen as unity, he regards the masses of the same volume of other volatile bodies as representing their molecular weight, and in the case of the elements he employs their symbols to indicate the molecules.

"'In water he considers that the molecule of hydrogen is combined with half a molecule of oxygen, forming one of steam, and he therefore represents the compound as $HO_{\frac{1}{2}}$. He does not make use of the term "atom" (although he speaks of atomic weight on p. 18, but thinks it divisible), and if he had called the smallest proportion of an element which enters into combination an atom, he would probably have been led to believe that the molecules of some of the simple bodies contain two atoms, and he might have adopted two volumes to represent the molecule, as is done at the present time. The author calls one volume or molecule of chlorine Cl, one volume or molecule of hydrogen H, and one volume or molecule of hydrochloric acid $H_{\frac{1}{2}}Cl_{\frac{1}{2}}$. If he had regarded the molecules as containing two indivisible atoms, these bodies would have been represented, as now, by the formulæ Cl_2, H_2, and HCl respectively, all occupying two volumes. § 15 shows how near he was to this conception. Gerhardt, in the fourth part of his "Traité de Chimie Organique," published in 1856, points out the uniformity introduced into chemical theory by the adoption of this system.

"'For carbon he makes C = 12, as now accepted, although I do not find how he arrives at this number. He seems to have anticipated one of Ramsay's recent discoveries, that nitrous anhydride (hyponitrous acid, $ON_{\frac{2}{3}}$, No. 26 in the table) dissociates on evaporation into nitric oxide (binoxide of nitrogen, No. 23) and nitric peroxide (nitrous acid, No. 25).

"'The values for the symbols for sulphur, phosphorus, and arsenic, taken from the vapour densities (and which are multiples of what are believed to be the true atomic weights), cause some complexity in the formulæ of their compounds.

"'There seem to be errors in the formulæ of alcohol and ether on p. 49, for they do not agree with those in the table. They ought probably to be written

$$2(HC_{\frac{1}{2}}) + O_{\frac{1}{2}}2H_{\frac{1}{2}} \text{ and } 4(HC_{\frac{1}{2}}) + O_{\frac{1}{2}}2H_{\frac{1}{2}}.$$

"'Considering how nearly Waterston approached what is now believed to be the true theory, it is disappointing to read his controversy with Odling in 1863 and 1864 (*Phil. Mag.*, vols. xxvi. and xxvii.), where he seems to oppose the new formulæ then being introduced. He is very dogmatic about the constitution of hydrate of potash: he very properly insists that we can only obtain a knowledge of the molecular weight of bodies that can be volatilized, and of which the vapour densities can be determined, but he does not see the analogy between the hydrate and oxide of potassium with alcohol and ether, probably because he regards these latter bodies as combinations of water with different quantities of olefiant gas. He writes water $HO_{\frac{1}{2}} = 9$, alcohol $CH_2HO_{\frac{1}{2}} = 23$, and ether $C_2H4 \, . \, HO_{\frac{1}{2}} = 37$, whilst he considers potassic hydrate $KO_{\frac{1}{2}} \, . \, HO_{\frac{1}{2}} = 56$, and oxide of potassium $KO_{\frac{1}{2}} = 47$, the hydrate having a higher molecular weight than the oxide. If we regard these compounds as derived from water by the replacement of hydrogen by ethyl and potassium respectively, the analogy between the two series is complete (ethyl was discovered in 1849, and is mentioned by Waterston).

$$\begin{array}{rlrl} H_2O &= 18 & H_2O &= 18. \\ (C_2H_5)HO &= 46 & KHO &= 56. \\ (C_2H_5)_2O &= 74 & K_2O &= 94. \end{array}$$

"'From a remark in the *Phil. Mag.* (vol. xxvi. p. 520), I imagined that Waterston had arrived at the double atomic weights of many of the metals now adopted, for he gives that of iron as 56 and that of aluminium as 27, calculated from their specific heats, but there is an error in his arithmetic, for 3·3 divided by the specific heat of iron ·1138 gives 28·998, and 3·3 divided by the specific heat of aluminium ·2143 gives 15·399.'

"With the exception of some corrections relating merely to stops and spelling, the paper is here reproduced exactly as it stands in the author's manuscript.—December 1891."

The author's own introduction to his memoir, which occupies eighty pages of the Philosophical Transactions as now printed, runs as follows:—

"Of the physical theories of heat that have claimed attention since the time of Bacon, that which ascribes its cause to the intense vibrations of the elementary parts of bodies has received a considerable accession of probability from the recent experiments of Forbes and Melloni. It is admitted that these have been the means of demonstrating that the mode of its radiation is identical with that of light in the quantities of refraction and polarization. The evidence that has been accumulated in favour of the undulatory theory of light has thus been made to support with a great portion of its weight a like theory of the phenomena of heat; and we are, perhaps, justified in expecting that the complete development of this theory will have a much more important influence on the progress of science, because of its more obvious connection and intimate blending with almost every appearance of Nature. Heat is not only the subject of direct sensation and the vivifier of organic life, but it is manifested as the accompaniment of mechanical force. It is related to it both as cause and effect, and submits itself readily to measurement by means of the mechanical changes that are among the most prominent indications of its change of intensity. The undulatory theory at once leads us to the conclusion that, inasmuch as the temperature of a body is a persistent quality due to the motion of its molecules, its internal constitution must admit of it retaining a vast amount of living force. Indeed, it seems to be almost impossible now to escape from the inference that heat is essentially molecular *vis viva*. In solids, the molecular oscillations may be viewed as being restrained by the intense forces of aggregation. In vapours and gases these seem to be overcome; vibrations can no longer be produced by the inherent *vis insita* of the molecules struggling with attractive and repellent forces; the struggle is over and the molecules are free; but they, nevertheless, continue to maintain a certain temperature; they are capable of heating and being heated; they are endowed with the quality heat, which, being of itself motion, compels us to infer that a molecule in motion without any force to restrain or qualify it, is in every respect to be considered as a free projectile. Allow such free projectiles to be endowed with perfect elasticity, and likewise extend the same property to the elementary parts of all bodies that they strike against, and we immediately introduce the principle of the conservation of *vis viva* to regulate the general effects of their fortuitous encounters. Whether gases do consist of such minute elastic projectiles or not, it seems worth while to inquire into the physical attributes of media so constituted, and to see what analogy they bear to the elegant and symmetrical laws of aëriform bodies.

"Some years ago I made an attempt to do so, proceeding synthetically from this fundamental hypothesis, and have lately obtained demonstration of one or two points where the proof was then deficient. The results have appeared so encouraging, although derived from very humble applications of mathematics, that I have been led to hope a popular account of the train of reasoning may not prove unacceptable to the Royal Society.—September 1, 1845."

REPORT OF THE ROYAL SOCIETY'S COMMITTEE ON COLOUR VISION.

A COMMITTEE, consisting of Lord Rayleigh as Chairman, Lord Kelvin, Mr. Brudenell Carter, Prof. Church, Mr. J. Evans, Dr. Farquharson, M.P., Prof. Michael Foster, Mr. Galton, Dr. Pole, Sir G. Stokes, and Captain Abney, as Secretary, was appointed by the Council of the Royal Society in March 1890, to consider the question of testing for defective colour vision. Their report has just been presented to the Royal Society, and it possesses great practical interest for all classes, considering that on the average one male out of every twenty-five suffers more or less from this form of blindness.

The Committee have taken evidence as to the tests in general use on the railways, and also as to those which have been for some time adopted by the Board of Trade for the mercantile marine service, and have supplemented it by carrying on practical examinations on their own account. Experts have also given evidence as to the different forms of colour-blindness to be found, and the fact that it may be induced by disease as well as be congenital has been brought prominently forward by Dr. Priestley Smith, of Birmingham, and Mr. Nettleship, of St. Thomas's Hospital, and we have it on their authority that this type is not a negligible one. As an outcome of their investigations, the Committee have unanimously agreed to the following recommendations:—

(1) That the Board of Trade, or some other central authority, should schedule certain employments in the mercantile marine and on railways, the filling of which by persons whose vision is defective either for colour or form, or who are ignorant of the names of colours, would involve danger to life and property.

(2) That the proper testing, both for colour and form, of all candidates for such employments should be compulsory.

(3) That the testing should be intrusted to examiners certificated by the central authority.

(4) That the test for colour vision should be that of Holmgren, the sets of wools being approved by the central authority before use, especially as to the correctness of the three test colours, and also of the confusion colours. If the test be satisfactorily passed, it should be followed by the candidate being required to name without hesitation the colours which are employed as signals or lights, and also white light.

(5) That the tests for form should be those of Snellen, and that they should be carried out as laid down in Appendix VI. It would probably, in most cases, suffice if half normal vision in each eye were required.

(6) That a candidate rejected for any of the specified employments should have a right of appeal to an expert approved by the central authority, whose decision should be final.

(7) That a candidate who is rejected for naming colours wrongly, but who has been proved to possess normal colour vision, should be allowed to be re-examined after a proper interval of time.

(8) That a certificate of the candidate's colour vision and form vision according to the appointed tests, and his capacity for naming the signal colours, should be given by the examiner; and that a schedule of persons examined, showing the results, together with the nature of the employments for which examinations were held, should be sent annually to the central authority.

(9) That every third year, or oftener, persons filling the scheduled employments should be examined for form vision.

(10) That the tests in use, and the mode of conducting examinations at the different testing stations, should be inspected periodically by a scientific expert, appointed for that purpose by the central authority.

(11) That the colours used for lights on board ship, and for lamp signals on railways, should, so far as possible, be uniform, and that glasses of the same colour as the green and red sealed pattern glasses of the Royal Navy, should be generally adopted.

(12) That in case of judicial inquiries as to collisions or accidents, witnesses giving evidence as to the nature or position of coloured signals or lights should be themselves tested for colour and form vision.

These recommendations have been framed after duly weighing all the evidence they have collected, and from the results of the experiments they have carried out during the last two years; and the reasons for adopting them are set forth at some length in the report. The Committee have, perhaps wisely, refused to endorse any particular hypothesis of colour vision, though they have described two, those of Young and Hering, in some detail, no doubt considering that everything which might be debatable had better be avoided when practical recommendations alone were in question. It is, however, a matter of some regret that this should be the case, as a Committee so strongly constituted should have been able, if not to convince every one, at least to lead opinion into proper channels. What little they have said in the notes to the report leads one to suspect that they are not satisfied that either Young or Hering has given a theory which will satisfy all requirements. Leaving, however, the question of theory, we may point out that the practical necessity of insisting, on the grounds of public safety, that certain posts on railways and on board ship should only be filled by persons possessing normal colour vision, no sane man would call in question. The peril that must arise, for instance, if an engine-driver could by any possibility mistake a red signal of danger for a green signal of safety, or if a look-out man on board ship should be liable to make a similar error, is self-evident; and it is to prevent any such risks being run that the Committee buckled to the task of recommending tests which should be efficient and perfectly trustworthy. There has been for a long time a suspicion, if not more than a suspicion, that the examinations carried on for colour vision by the Board of Trade in the mercantile marine were inadequate in both respects; and what little was known regarding the tests employed by the various railway companies engendered the same feeling of distrust, in those who had considered the subject in a scientific spirit. The evidence shows that the Board of Trade examiners have passed on a second